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THE LOGIC OF DISCOVERY

INTRODUCTION

F. C. S. SCHILLER distinguished the Logic of Proof from the Logic of Discovery.[1] No one, not even Schiller, has been clear about what was intended here. He probably felt his colleagues were too busy sectioning syllogisms—ignoring inferences which mattered in science. He would have approved of the attention philosophers now lavish on inductive inference, and the so-called "hypothetico-deductive method." (For this abominable appellation I substitute "H-D method.") This H-D kind of inquiry would have met Schiller's demand for a study of the Logic of Discovery. Nonetheless, I doubt whether this H-D method has anything to do with real discovery in natural science.

The Logic of Proof (i.e., deductive logic) has claimed philosophers' attention more than the Logic of Discovery. And even the latter, with its analyses of inductive reasoning, the foundations of probability, and the principles of theory-construction,—all this reads less like a Logic of Discovery than like a Logic of the Finished Research Report. Logicians of science have described how one might set out reasons in support of an hypothesis once it is proposed. They have said little about the conceptual considerations pertinent to the initial proposal of an hypothesis. There are two exceptions: Aristotle and Peirce. When they discussed what Peirce called "retroduction," both recognized that the proposal of an hypothesis is often a reasonable affair. One can have good reasons, or bad, for suggesting an hypothesis initially. These may be different from reasons which lead one to accept the hypothesis once suggested; in some cases the two may be different in type. This is not to deny that sometimes one's reasons for proposing an hypothesis are identical with his reasons for accepting it.

Neither Aristotle nor Peirce imagined himself to be setting out a manual to help scientists make discoveries. There could be no

[1] Cf. *Studies in the History and Methods of the Sciences*, ed. by Charles Singer.

such manual.[2] Nor were they discussing the psychology of discoverers, or the sociology of discovery. There are such discussions,[3] but they are not logical discussions. Aristotle and Peirce were doing logic. They examined characteristics of the reasoning behind the original suggestion of certain hypotheses. And this is what I should like to do.

I

A. I begin therefore by distinguishing

(1) reasons for accepting an hypothesis H, from
(2) reasons for suggesting H in the first place.

Some philosophers will deny that there is any *logical* difference between these two:—this must be faced. But let the distinction be drawn out boldly first.

What are reasons for accepting H? They are just those one might have for thinking H true. But the reasons for suggesting H originally, or even for formulating H, may not be those one would require before thinking H true. They are, rather, those reasons which make H *a plausible conjecture*. No one denies *some* differences between deciding that H is true, and deciding that H is a plausible conjecture. The question is, are these really logical, or more properly called "psychological" or "sociological"?

Thus H will be accepted as true if repeated observations support H—if consequences of H, used as predictions, confirm H—if new phenomena are revealed through operations on H. Again, if H is compatible with, or derivable from, already established theories, this will incline us to accept H as true.

Clearly, all this need not be considered before formulating H as a plausible conjecture. Sometimes one's reasons for proposing that all α's are β's may *just* be that all the α's ever observed were also β's: this will also be his reason for accepting that all α's are β's. But not all cases are like this. Kepler, e.g., could not have set out all the reasons which ultimately supported his elliptical orbit hypothesis *before* the idea of such an orbit for Mars seized him as an at least plausible possibility.

Considerations relevant to Kepler's hypothesis being a plausible possibility, however, might be these:

(1) Does H look as if it might be that from which known phenomena $p_1, p_2 \ldots$ etc., could be shown to follow?

[2] "There is no science which will enable a man to bethink himself of that which will suit his purpose" (J. S. Mill, *A System of Logic*, Book III, Ch. 1, § 2).

[3] E.g., those of Hadamard and Poincaré, Helmholtz and Beveridge.

(2) Does H look as if it might explain p_1, p_2 . . . etc.? (For some values of H the answer at any time would be "No"—an answer for which good reasons could usually be marshaled.)

Or, to follow through with Kepler's *De Motibus Stellae Martis:*

(1) Does the hypothesis of a non-circular orbit for Mars appear to be that from which it would follow that the planet's apparent velocities at 90° and at 270° of eccentric anomaly would be greater than if the orbit were circular? and
(2) Does this hypothesis look as if it might *explain* these facts?

These queries were certainly relevant to Kepler's first formulations of the elliptical orbit idea.[4] And that these considerations are reasonable is seen, in a negative way, from the fact that other values of H (e.g., that Mars' *color* is responsible for its high velocities—or that the dispositions of Jupiter's moons are responsible) these would *not* have looked to Kepler as if they would imply or explain the surprising phenomena in question. He would have thought it unreasonable to attempt to develop them; he could have produced good arguments in favor of abandoning them.[5]

B. Now I shall be challenged. Some philosophers will mark as spurious my distinction between reasons for suggesting H and reasons for accepting H.[6] There may be "psychological" factors, they concede, which make certain hypotheses "look" as if they may explain and imply phenomena. Thus Ptolemy knew just as well as did Aristarchus before him and Copernicus after him, that heliocentrism was theoretically simpler, and technically easier to manage, than the hypothesis of a geocentric, geostatic universe. But, say my challengers, for psychological, sociological, or historical reasons, heliocentrism did not "look" to Ptolemy as if it could imply (much less explain) the absence of stellar parallax. This cannot be a matter of logic because for Copernicus heliocentrism did "look" as if it could explain this phenomenon. Insofar as scientists have *reasons* for formulating and entertaining hypotheses (as opposed to having hunches, shrewd suspicions, and inspired intuitions), these are *just* the reasons which show the hypothesis to be true. Thus, if the absence of stellar parallax is thought to constitute more than a psychological reason for Ptolemy's refusal

[4] Cf. *De Motibus Stellae Martis* (Munich), pp. 250 ff.

[5] Braithwaite's view differs: "But exactly which hypothesis was to be rejected was a matter for the 'hunch' of the physicist" (*Scientific Explanation*, p. 20).

[6] Reichenbach writes that philosophy "cannot be concerned with the first, but only with the latter" (*Experience and Prediction*, p. 382).

to entertain heliocentrism, then insofar it is equivalent to his reason for rejecting that hypothesis *as false*. Conversely, his reason for developing the geostatic hypothesis (namely, this absence of stellar parallax) *was* his reason for accepting that hypothesis as true. Again, Kepler's reasons for rejecting Mars' color or Jupiter's moons as responsible for the former's apparent accelerations—if he had reasons and was not simply guessing—were reasons which inductively supported (and also served as reasons finally for accepting as true) the non-circularity hypothesis.

So the objection to my distinction between reasons for proposing H and reasons for accepting H comes to this: *the only logical reason for proposing H at all is that certain considerations incline one to think H true. Obviously these are the same considerations which (if substantiated) will ultimately lead one to account H as true. The distinction Hanson advocates is at bottom merely psychological, sociological, or historical in nature; it says nothing of logical import about the differences between suggesting and establishing scientific hypotheses.*[7]

Kepler may again illustrate the objection. No historian of planetary theory would deny that the principle of uniform circular motion profoundly affected astronomical thought before 1600. Even young Kepler in 1591 abandons an hypothesis because it entails other-than-uniform-circular motions in the heavens—inconceivable for him. Psychological pressure against forming alternative hypotheses was therefore great. But *logically* Kepler's reasons for entertaining a Martian motion other than uniformly circular were precisely his reasons for accepting that idea as an astronomical truth. He first encountered this hypothesis on perceiving that no simple adjustment of the orthodox epicycle, deferent, and eccentric could square with Mars' observed distances, velocities, and apsidal positions. These were the first reasons which led him ultimately to assert that the planet's orbit is not the effect of uniform-circular motions. Even after scores of other inductive reasons confirmed the truth of the non-circularity hypothesis, these early reasons *were still* reasons for accepting H as true. So they cannot have been merely reasons for proposing H and nothing more.

This objection is made as strong as possible. If the arguments I now bring forward cannot weaken it essentially, my purpose will not have been achieved.

[7] Or, as Braithwaite puts it, "The solution of these historical problems involves the individual psychology of thinking and the sociology of thought. None of these questions are our business here." (*Scientific Explanation*, pp. 20–21.)

C. It still seems to me that in some cases one's reasons for entertaining an hypothesis and his reasons for accepting it are logically different. When Kepler published *De Motibus Stellae Martis* he had completely established that Mars' orbit was an ellipse, inclined to the ecliptic, the sun being in one of the foci. In the *Harmonices Mundi* (1619) Kepler generalized this for *all* the planets. Let us call this general hypothesis H′.

The reasons which led Kepler to formulate and propose H′ were many. But they certainly included this one: that H (the limited hypothesis that *Mars'* orbit is elliptical) is true. Since Eudoxos, Mars had been taken to reveal typical planetary behavior. (*We* can see why this would be so. Mars' retardations and retrogradations, its movement at perihelion and around the empty focus—all these we observe particularly clearly because of the actual spatial relations of Earth and Mars.) In a sense Mars is a typical planet. Its dynamical properties are usually found in the other planets. If *its* orbit is ellipsoidal, then it is reasonable to conjecture that the orbits of Mercury, Venus, Jupiter, Saturn, and even Earth, are also ellipsoidal—i.e., it is reasonable to entertain, develop, and suggest H′.

But these reasons would not *establish* the truth of H′. Because what makes it reasonable to propose H′ is *analogical* in character. (Mars does *x;* Mars is a typical planet; so (perhaps) all planets do *x*.) Analogies cannot establish hypotheses, only observations can: in this the H-D account is correct. To establish H′ one must observe the positions of the other planets, determining that each could be plotted on a smooth curve whose equations approximate to those of an ellipse. When this is done it may be possible to assert H′. But this could not have been done, nor would it be reasonable to expect it, before H′ was even proposed initially. Nor is it correct to characterize this difference between H′-as-proposed and H′-as-established as being one of psychology only (as Reichenbach and Braithwaite, among others, seem prepared to do). *Logically,* Kepler's analogical reasons for proposing H′ just after 1609 were good ones. But, logically, they would not have been good reasons for asserting the truth of H′—something which could be done confidently only years later. Remember, if the statement "A is a good reason for H" is true, it is logically true—even when the reasons are inductive. If I say "Jones *has* good reason for H," that is contingently true, if true at all. Jones could have had other reasons for H. But this statement is logically different from "A *is* a good reason for H." What are and are not good reasons is a logical matter. No further observations are required to settle

the matter—any more than we require experiments to decide, on the basis of all of Jones' bank statements, whether he is bankrupt. Similarly, whether or not A is a good reason for H's proposal is a purely logical inquiry.

It seems clear, then, that the difference between reasons which make it plausible to propose H and those which establish H is greater than is conveyed by calling them "psychological." Consider how reasoning proceeds from analogies on the one hand, and from observations on the other.

Kepler reasoned initially by analogy. Other kinds of reasons which might make it plausible to propose an hypothesis could be, e.g., the detection of a single formal structure, or symmetry, in sets of equations or arguments; this marked the work of Clerk Maxwell and Einstein. One could even argue reasonably from authority. Kepler's assistant, Bartsch, had a good reason for proposing H′: it was that Kepler also proposed H′. Just because a man proposes an hypothesis in "me too" fashion does not make his proposal unreasonable. It all depends on whom he takes his cue from. Even the purest of mathematicians rarely work out their own logarithmic tables. But these considerations would not establish H′. So reasons for proposing H′ and for accepting it are not only different, they can be different in type. Consider this further.

Reasoning from observations of α's as β's to the proposal of "all α's are β's" is different in type from reasoning analogically from the fact that γ's are δ's to the proposal of "all α's are β's." (Here it is the *way* in which γ's are α's which seems analogous to the way in which α's are β's.) Both of these are typically different from reasons consisting in the detection of symmetries in equations which describe α's and those describing β's. Again, all these differ from what can also be good reason for proposing that all α's are β's, namely, that all authorities are convinced that α's are β's. Microbiologists, e.g., accept the atomic hypothesis, and with good reason. Yet they may discover further reasons for accepting the hypothesis, e.g., the Brownian movement, and the properties of semi-permeable membranes.

SUMMARY

I have distinguished reasons for proposing hypotheses from reasons for accepting them. In some cases the two are identical; in others they can differ in type. There is a difference in type between proposing H as (1) a result of analogical argument, and (2) as a result of symmetry considerations, and (3) by appealing to authority—and between all of these and (4) the acceptance of H because of inductive arguments from observed particulars.

But here an objection obtrudes: "Analogical, authoritative, and symmetry arguments are used in certain cases *because* of inductively established beliefs in their reliability."

This I accept. However, I am not discussing the genesis of our faith in these types of arguments, but rather the logic of the arguments themselves. *Given* an analogical premise, or one based on authority or on symmetry—or indeed on enumeration of particulars,—one argues from these in logically different ways. This is clear once one considers what further moves would be necessary to convince one who doubted the conclusions of such arguments. A challenge to the conclusion "all α's are β's" when this is based on induction by enumeration, could only be a challenge to justify induction, or to show that the particulars are correctly described. These are inappropriate when the argument rests on analogies or on formal symmetries.

Another potent objection is this: "Analogical reasons, and those based on symmetry considerations and on appeals to authorities—all these are *still* reasons for H even after H is (inductively) established. They are thus reasons both for proposing *and* for accepting H." This is also accepted. Nonetheless, analogical, symmetry, and authoritative arguments could never *by themselves* establish an H. Inductive arguments can, by themselves, do this. So they must be different in type. Though any of these could make it reasonable to propose an H, only the inductive argument can by itself establish hypotheses.

Since H-D philosophers have been most articulate on these matters I must draw out a related issue on which their accounts are unsatisfactory.

II

J. S. Mill was wrong in his account of Kepler's discovery.[8] It is impossible to reconcile the labors of the *Astronomia Nova,* and the delicate adjustment between theory, hypothesis, and observation recorded in *De Motibus Stellae Martis,* with Mill's irresponsible statement that Kepler's First Law is just "a compendious expression for the one set of directly observed facts." Mill cannot have understood Kepler.[9] *A System of Logic* is as misleading about

[8] *A System of Logic,* Bk. III, Ch. 2–3.

[9] As Peirce notes, *Collected Papers,* I, p. 31. It is equally questionable whether Reichenbach understood Kepler: "Kepler's laws of the elliptic motion of celestial bodies were inductive generalizations of observed facts. . . . [He] observed a series of . . . positions of the planet Mars and found that they may be connected by a mathematical relation . . ." (*Experience and Prediction,* p. 371).

scientific research and discovery as any account which proceeded *via* what Bacon called "Inductio per enumerationem simplicem, ubi non reperitur instantia contradictoria."[10] In another way the accounts of H-D theorists are equally misleading.

An H-D account of Kepler's First Law would treat it as a high-level hypothesis in an H-D system. It would be regarded as a quasi-axiom, from the assumption of which observation-statements can be shown to follow. If these are true—if, e.g., they imply that Uranus' orbit is an ellipse and that its apparent velocity at 90° will be greater than at aphelion,—then insofar is the First Law confirmed.[11]

Perhaps this describes physical theory more adequately than did pre-Baconian accounts in terms of simple enumerations, or even post-Millian accounts in terms of ostensibly not-so-simple enumerations. It tells us something about the logic of laws, and what they do in the finished arguments of physicists. H-D accounts do not, however, tell us how laws are proposed in the first place—nor were they intended to. (Nonetheless those who deny real differences between reasons for suggesting H and reasons for accepting it are also inclined sometimes to suppose that scientific discovery *actually proceeds via* the industrious employment of the H-D method.)

Though the H-D account of scentific theories, laws, and hypotheses does not tell us in what kinds of rational contexts laws and hypotheses are suggested, the induction-by-enumeration story did attempt this. It sought to describe good reasons for initially proposing H. The H-D account ought to be silent on this point. The two accounts are not strict alternatives.[12] The induction-by-enumeration account and the H-D account are compatible. Acceptance of the second is no good reason for rejecting the first. A law *might* have been arrived at, or inferred from, little more than an enumeration of particulars (e.g., Boyle's Law in the 17th century,

[10] Reichenbach observes: "It is the great merit of John Stuart Mill to have pointed out that all empirical inferences are reducible to the *inductio per enumerationem simplicem* . . ." (*op. cit.*, p. 389).

[11] Thus Braithwaite writes: "A scientific system consists of a set of hypotheses which form a deductive system . . . arranged in such a way that from some of the hypotheses as premises all the other hyptheses logically follow. . . . The establishment of a system as a set of true propositions depends upon the establishment of its lowest-level hypotheses . . ." (*Scientific Explanation*, pp. 12–13).

[12] As Braithwaite suggests they are when he remarks of a certain "higher-level" hypothesis that it "will not have been established by induction by simple enumeration; it will have been obtained by the hypothetico-deductive method . . ." (*op. cit.*, p. 303).

Bode's Law in the 18th, the Laws of Ampère and Faraday in the 19th, and much of meson theory in the 20th). It could then be built into an H-D system as a higher-order proposition—as indeed has happened with all my examples save the last. So, if there is anything wrong with the older view, H-D accounts do not reveal what this is.

There is something wrong with the older accounts. They are false. Scientists do not always find laws by enumerating and summarizing observables.[13] But this does not strengthen the H-D account of the matter as against the inductive view. There is no H-D account of how "sophisticated generalizations" are *derived.*

If the H-D account were construed as a description of scientific practice it would be misleading.[14] Natural scientists do not "start from" hypotheses. They start from data. And even then not from ordinary commonplace data—but from surprising anomalies. Thus Aristotle remarks[15] that knowledge begins in astonishment. Peirce makes perplexity the trigger of scientific inquiry.[16] And James and Dewey treat intelligence as the result of mastering problem situations.[17]

By the time a law gets fixed into an H-D system, the *original* scientific thinking is over. The more pedestrian process of deducing observation-statements begins only after the physicist is convinced that the proposed hypothesis will at least explain the data initially requiring explanation. Thus Kepler's assistant could easily work out the consequences of H′, and check its validity by seeing whether Mercury, Venus, Earth, Jupiter, and Saturn behaved as H′ predicts. This was possible because of Kepler's reasonable conviction that what H had done for Mars, H′ would do for the other planets. The H-D account is helpful here; it analyzes *the argument of a completed research report,* such as Bartsch's report that the consequences of H′ square with the observed positions of the planets. It helps us also to see how the experimentalist elaborates a theoretician's hypotheses. And yet another aspect of science the H-D account illuminates, but its proponents oddly have

[13] Thus Braithwaite says: "sophisticated generalizations (such as that about the proton-electron constitution of the hydrogen atom) . . . [were] certainly not derived by simple enumeration of instances . . ." (*op. cit.,* p. 11).

[14] Braithwaite's use of "derived" is thus misleading. So is his announcement (p. 11) that he is going to explain "how we *come to make use* of sophisticated generalizations."

[15] *Metaphysica* 982 b 11 ff.

[16] *Collected Papers,* II, Book III, Ch. 2, Part III.

[17] Dewey, *How We Think,* pp. 12 ff.

not stressed it; scientists often dismiss explanations alternative to the one which has won their provisional assent in a way that is almost a model of the H-D method in action. Examples of this are in Ptolemy's *Almagest*, when he rules out a moving earth; in Copernicus' *De Revolutionibus* . . . , when he demolishes Ptolemy's lunar theory; in Kepler's *De Motibus Stellae Martis*, when he denies that the planes of the planetary orbits intersect in the center of the ecliptic instead of (as he proposed) the center of the sun; and in Newton's *Principia*, when he rejects the idea that the gravitational force law might be of an inverse cube nature. These mirror parts of Mill's *System of Logic*, or Braithwaite's *Scientific Explanation*.

Notwithstanding these merits, however, the H-D analysis leaves undiscussed reasoning which often conditions the discovery of laws.

The induction-by-enumeration story views the important inference as being from observations to the law, from particulars to the general. There is something true about this which the H-D account must ignore. Thus Newton wrote: "the main business of natural philosophy is to argue from phenomena. . . ."[18]

This inductive view ignores what Newton never ignored; the inference is also from *explicanda* to an *explicans*. Why a beveled mirror shows a spectrum in the sunlight is not explained by saying that all beveled mirrors display spectra in sunlight. Why Mars moves more rapidly at 270° and 90° than could be expected of circular-uniform motions is not explained by saying that Mars always moves in this manner—or even that all the planets always move in this manner. On the induction-by-enumeration view, these latter might count as laws. But clearly, only when it is explained why beveled mirrors show spectra in the sunlight, and why planets apparently accelerate at 90°—only then will we have laws of the type suggested: Newton's Laws of Refraction and Kepler's First Law.

So the inductive view rightly suggests that laws are got by inference from data. It wrongly suggests that the law is but a summary of these data, instead of being (what it at least sometimes must be) an explanation of the data.

H-D accounts all agree that physical laws explain data.[19] However, they obscure the initial connection between data and laws.

[18] *Principia*, Preface.

[19] Thus Braithwaite says: "A hypothesis to be regarded as a natural law must be a general proposition which can be thought to *explain* its instances; if the reason for believing the general proposition is solely direct knowledge of the truth of its instances, it will be felt to be a poor sort of explanation of these instances . . ." (*op. cit.*, p. 302).

Indeed, they often suggest that the fundamental inference in science is from higher-order hypotheses to observation-statements. This may be a way of setting out reasons for making a prediction after H is formulated and provisionally established. It need not be a way of setting out reasons in favor of proposing H originally. Bartsch could have justified a prediction that Saturn will appear to move faster at 270° and 90° than the hypothesis of its uniform circular motion indicates. Referring to H′ and its success with the other planets, he would show how this conclusion about Saturn is entailed by H′. But he would not have set out thus his reasons for entertaining H′ initially—if he had any reasons other than that Kepler was convinced that H′ was true. Certainly Kepler himself would not have set out thus his reasons for proposing H originally.

Yet the original suggestion of an hypothesis is often a reasonable affair. It is not as dependent on intuition, hunches, and other imponderables as historians and philosophers suppose when they make it the province of genius but not of logic. If the establishment of H through its predictions has a logic, so has the argument which leads to H's proposal initially. To form the first idea of an elliptical planetary orbit, or of constant acceleration, or of universal gravitational attraction does indeed require genius; nothing less than a Kepler, a Galileo, or a Newton. But this need not entail that reflections leading to these ideas are unreasonable, or a-reasonable. Perhaps *only* Kepler, Galileo, and Newton had intellects mighty enough to fashion these notions initially. To concede this is not to concede that their reasons for first entertaining such concepts surpass rational inquiry.

H-D accounts begin with the hypothesis as given, as cooking recipes begin with the trout as given. In an occasional ripple of culinary humor, however, recipes sometimes begin with "First catch your trout." The H-D account describes a recipe physicists often use after catching hypotheses. However, the ingenuity and conceptual boldness which mark the whole history of physics show more clearly in the ways in which scientists *caught* their hypotheses, than in the ways in which they elaborated these once caught.

To study only the verification of hypotheses is to leave a vital part of the story untold—namely, what were the reasons Kepler, Galileo, and Newton had for suggesting their hypotheses initially. In a letter to Fabricius, Kepler underlines this distinction.

Prague, July 4, 1603

Dear Fabricius,

. . . You believe that I start with imagining some pleasant hypothesis and please myself in embellishing it, examining it only later by observations. In this you are very much mistaken. The truth is that after having built up

an hypothesis on the ground of observations and given it proper foundations, I feel a peculiar desire to investigate whether I might discover some natural, satisfying combination between the two. . . .

If any H-D theorist ever sought to give an account of the way in which hypotheses in science are actually discovered, these words are for him.

III

Have any H-D theorists ever thought that they were doing this? Some have. This is connected with their denial of any logical distinction to be made between reasons for proposing H and reasons for accepting it.

Braithwaite writes: "every science *proceeds* . . . by thinking of general hypotheses . . . from which particular consequences are deduced which can be tested by observation . . ."[20] (compare Kepler's letter) and "science proceeds (by the hypothetico-deductive method)"[21]; and again, "Galileo's deductive system was . . . presented as deducible from . . . Newton's Laws of Motion and . . . his Law of Universal Gravitation. . . ."[22] (N.B. An ingenious theologian could show Galileo's system as "deducible from" certain higher-level propositions about God's attributes. What matters is not that it is *deducible* from God's attributes, or Newton's Laws, but that it is *explained by* these Laws. Known planetary positions were deducible by Ptolemy from circular motion, epicycle, deferent, and eccentric. But these positions were not explained *via* such techniques. This Ptolemy conceded, as did Hipparchus and Apollonius before him, and Thomas and Dante after.)

Rashevsky sees Newton's discovery of universal gravitation as the result of employing the H-D method as a method of inquiry. He writes:

> . . . the scientist . . . studies *in abstracto* all . . . conceivable explanations. Then, by comparing the results of his studies with actual observation, he decides which of the possible explanations apply best. . . . First Newton formulated his general laws of motion, which describe the motion of bodies under the influence of any kind of force. Next he studied the motion under different kinds of central forces. It was then found that the actual motion of planets . . . should occur under the influence of a central force which varies as the inverse square of the distance. The conclusion was thus reached that the force of gravitation varies with distance in this manner.[23]

[20] (*Op. cit.*,) p. ix. (my italics)
[21] *Op. cit.*, p. xi.
[22] *Op. cit.*, p. 18.
[23] *Mathematical Thinking in the Social Sciences*, ed. by Lazarsfeld, p. 69.

How one can be misled by H-D accounts of the development of scientific thought, impinges here on the history of physics. On the analysis of this event hangs the plausibility of my position.

An H-D analysis of the law of gravitation, as established in mechanics, would be this one:

1. First the hypothesis H: that between any two particles in the universe exists an attracting force varying inversely as the square of the distance between them ($F = \gamma Mm/r^2$).
2. Deduce from this (in accordance with the *Principia*)
   a. *Kepler's* Laws, and
   b. *Galileo's* Laws.
3. But particular instances of *a.* and *b.* square with what is observed.
4. Therefore H is, insofar, confirmed.

The H-D account says nothing about the context of reasoning in which H was first puzzled out. If (with Rashevsky) it tries, it goes wrong, as I shall argue. But first I suggest why here the H-D account is *prima-facie* plausible.

Historians often remark that Newton's serious reflections on this problem began in 1680 when Halley asked him this question: If between a planet and the sun there exists an attraction varying inversely as the square of their distance, what then would be the path of the planet? Halley was astonished with the immediate answer: "An ellipse." The astonishment arose not because Newton *knew* the path of a planet, but because it appeared that he had managed to deduce this from the hypothesis of universal gravitation. Halley begged for the proof; but it had been long since lost in the chaos of Newton's untidy room. The promise to work it out anew eventually terminated in the writing of the *Principia* itself. Thus the story unfolds along the lines of the H-D schema: (1) from the suggestion of an hypothesis (whose genesis is a matter of logical indifference, i.e., psychology, or sociology) to (2) the deduction of observation statements (the laws of Kepler and Galileo)—which turn out true, thus (3) establishing the hypothesis.

Another thing which supports the H-D account is this: the *Principia* actually unfolds as the schema requires—from propositions of high generality to those of restricted generality, terminating in observation-statements which are true. Thus Braithwaite observes:

> Newton's *Principia* [was] modelled on the Euclidean analogy and professed to prove [its] later propositions—those which were confirmed by confrontation with experience—by deducing them from original first principles. . . .[24]

[24] *Scientific Explanation*, p. 352.

Despite this plausibility I long ago became suspicious of this orthodox account. The answer which Newton is reported to have given to Halley is not unique. He could have said "a circle" or "a parabola," and have been equally correct—or equally incorrect. The general answer to Halley's question is: "A conic section." The greatest mathematician of his day is not likely to have dealt with so obviously mathematical a question—requiring to know whether a certain formal demonstration is possible—with an answer which is no more than a single value of the correct answer.

Yet the inference in the reverse direction, that is, the retroduction, *is* unique. Given that the planetary orbits are ellipses, and allowing Huyghen's law of centripetal force, and Kepler's rule (that the square of a planet's mean period of revolution is proportional to the cube of its mean distance from the sun)—from this the law of gravitation can be inferred uniquely. Thus, given the question, "If the planetary orbits are ellipses what form will the force law take?," the unique answer is "an inverse square law."

From the fact of the elliptical orbit, one can (by way of Huyghen's Law and Kepler's third law) *explain* this uniquely by showing it to follow as a matter of course from the less well-known law of universal gravitation.

The idea behind these proofs is roughly as follows: Given an egg-shell, elliptical in section (rather than oviform), imagine a marble moving inside that shell (with velocity sufficient to keep it in the maximum elliptical orbit). What force must the egg-shell exert on the marble to keep the latter in this path? Huyghen's weights, when whirled on strings, required a force in the string, and in Huyghen's arm, of $F_{(k)} = r/T^2$ (where r signifies distance, T time, and k is a constant of proportionality). This restraining force was needed to keep the weights from flying away like catapulted projectiles, and something like this force would thus be expected in the egg-shell. But from Kepler's third law we know that $T^2 = r^3$. Hence $F_{(k)} = r/r^3 = 1/r^2$. The force the shell exerts on the marble varies inversely as the square of the distance of the marble from that focus of the ellipse in which the sun rests. This follows by retroductive reasoning. But what *is* this retroductive reasoning whose superiority over the H-D account has been hinted at throughout this paper?

Schematically, it can be set out thus:

1. Some surprising, astonishing phenomenon p_1, p_2, p_3 . . . is encountered.[25]

[25] The astonishment may consist in the fact that p is at variance with accepted *theories*—as, e.g., the discovery of discontinuous emission of radiation

2. But p_1, p_2, p_3 . . . would not be surprising or astonishing if H were true—they would follow as a matter of course from H; H would explain p_1, p_2, p_3 . . . .
3. Therefore there is good reason for elaborating H—for proposing it as a possible hypothesis from whose assumption p_1, p_2, p_3 . . . might be explained.

This presentation is a free development of remarks in Aristotle [26] and Peirce.[27]

How, then, would the discovery of universal gravitation fit into such an account?

1. The surprising, astonishing discovery that all planetary orbits are elliptical was made by Kepler (1605 to 1619).
2. But such an orbit would not be surprising or astonishing if, in addition to other familiar laws, an inversely varying law of gravitation obtained. Kepler's first law would follow as a matter of course; indeed the hypothesis could even explain why (since the sun is in but one of the foci) the orbits are ellipses on which the planets travel with non-uniform velocity.
3. Therefore there is good reason for elaborating this hypothesis further, for proposing it as that from the assumption of which Kepler's first law might be explained.

This tells us something about the rational context within which H might come to be "caught" in the first place. It begins where all physics begins—with problematic phenomena requiring explanation. It suggests what might be done to hypotheses once proposed, namely, the H-D elaboration. And it points up how much philosophers of science have yet to learn about the kinds of reasons scientists might have for thinking a particular hypothesis may explain initial perplexities—why, e.g., one hypothesis may be preferred over others if it throws the data into patterns within which determinate modes of connection can be perceived. These are the problems which Wittgenstein unearths in the *Philosophical*

by hot black bodies, or the photoelectric effect, the Compton effect, and the continuous β-ray spectrum—or the orbital aberrations of Mercury, the refrangibility of white light, and the high velocites of Mars at 90°. What is important here is *that* the phenomena are encountered as anomalous, not *why* they are so regarded.

[26] *Prior Analytics*, II, 25.

[27] *Collected Papers*, Vol. I, § 188. Peirce amplifies this: "It must be remembered that retroduction, although it is very little hampered by logical rules, nevertheless is logical inference, asserting its conclusion only problematically, or conjecturally, it is true, but nevertheless having a perfectly definite logical form."

Investigations and which open up unexplored aspects of scientific research. At least it appears that the ways in which scientists sometimes reason their way towards hypotheses may be as legitimate an area for conceptual inquiry as are the ways in which they sometimes reason their way from hypotheses.

My distrust of the H-D account of Newton's work—which either says nothing about what led Newton to propose universal gravitation, or else mistells the whole story—has been reinforced by manuscripts in the Lord Portsmouth collection in the Cambridge University Library. There, in "Additional manuscripts 3968, No. 41, bundle 2," is the following draft in Newton's own hand:

> . . . And in the same year [1665, twenty years before the *Principia*] I began to think of gravity extending to ye orb of the Moon, and (having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere), from Kepler's rule . . . I deduced that the forces which keep the planets in their Orbs must be reciprocally as the squares of their distances from the centres about which they revolve. . . .

This MS., though familiar to some historians, was unknown to me in writing the preceding. It corroborates my earlier conjecture as to the real reasoning behind Newton's proposal of this hypothesis. ("Deduce," in this passage, is used as when Newton speaks of deducing laws from phenomena—which is just what Aristotle and Peirce would call "retroduce.") So, Newton knew how to estimate the force of a small globe on the inner surface of a sphere. (To compare this with Halley's question and with my earlier tentative reconstruction, note that such a sphere can be regarded as a degenerate solid ellipse, i.e., where the foci superimpose). From this, and from Kepler's rule $T^2 = r^3$, Newton retroduced the law of gravitation as that by reference to which these other things would be entailed and, more importantly, would be explained. These were the reasons, the good reasons, which led Newton to think further on universal gravitation. The reasons for accepting this hypothesis as a law are powerfully set out in the *Principia* itself—and they are much more comprehensive than anything which occurred to him at this early age.

My task has been to illuminate an aspect of scientific inquiry neglected by philosophers, namely, the scientists' reasons for considering hypotheses seriously in the first place. Some philosophers, the inductionists (Bacon, Locke, Hume, Mill, Reichenbach), have given a wrong answer to this, suggesting, e.g., that all scientists reason initially from phenomena to hypotheses by variations on "inductio per enumerationem simplicem." They make it sound as if insight and genius have nothing to do with discovery. Other philosophers (Lenzen, Braithwaite, Reichenbach), H-D theorists,

say nothing about the matter—but sometimes because they deny any logical differences between the Logic of Discovery and the Logic of Proof. They make it sound as if logic and reason have nothing to do with discovery. Still other H-D thinkers do discuss reasons for entertaining hypotheses initially, but get the issue twisted, putting hypotheses where the surprising phenomena should be. Aristotle, as usual, carves out the important trail here, and Wittgenstein, as usual, lights up the crossroads. More philosophers must venture into these unexplored regions in which the logical issues are often hidden by the specialist work of historians, psychologists, and the scientists themselves. We must attend as much to how scientific hypotheses are caught, as to how they are cooked.

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COMPLEMENTARITY AND ITS ANALOGIES

I

BOHR'S principle of complementarity represents an attempt to escape from the untenable position, in which the new physics was involved, of having to attribute contradictory properties to objects. Certain peculiarities of sub-atomic observation and theoretical considerations seemed to lead to this position. It was found that it was not possible to observe entities of a certain order of magnitude without the act of observation introducing a significant effect upon the entity's behavior. As is expressed in the Heisenberg Indeterminacy Principle, this experimental interaction places a theoretical limit upon the accuracy with which simultaneous measures of space-time co-ordinates and momentum-energy components can be obtained. At the limit, perfectly accurate measure of one set of variables obviates any measure whatever of the other set. Interpretation of experimental data thus limited can be constructed either in terms of a classical wave theory or in terms of a classical particle theory, depending upon the specific nature of the experimental set-ups. The odd and initially bewildering feature of this state of affairs was that it led to the assertion that the same entity could be both wave-like and particle-like. Since these two representations are in classical terms mutually exclusive, the experimental results led to assertions about a single object which were contradictory.

It is often said at this point that once the de Broglie waves, Schrödinger functions, and other abstract symbolizations had been

developed and we could talk in terms of such things as the probability distribution of a particle in a wave, we could dispense with the troublesome classical interpretations and altogether bypass the problem by depending upon non-representational, abstract mathematical interpretations. But Bohr has this to say: "However far the phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms. . . . The account of the experimental arrangement and of the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics."[1] That is to say that there are some points at which we cannot (by the nature of experiment) dispense with the classical concepts and that this is just at the point of our experimental contact with the objects. In other words, whenever we must check theory against experiment we are tied to the classical concepts because only insofar as we can contrive our experiments so as to obtain clear yes-no answers in individual cases (a light flashing on or off, etc.), where we can trace some kind of causal connections in our perceptual experience, can we have knowledge of the objects under study. Thus, if we are to attain any kind of rational interpretation of phenomena which are quantized in nature, discrete or discontinuous, we can get to it only by mediating these effects through experimental set-ups which give us the kind of yes-no answers we require. It is only experimental set-ups constructed along classical lines that can yield such results, no matter what alternative to classical description the phenomena may force us to adopt.

Bohr's principle of complementarity, then, is to reconcile the *prima facie* incompatibility of the two classical representations to which we are forced by the inescapable dependency upon classical terms under which we operate in the observation of objects. Bohr's argument for the complementarity and not contradictoriness of the particle and wave representations rests upon the fact that phenomena appropriate to the two kinds of description can never arise under the same experimental conditions, but only under mutually exclusive arrangements. Therefore, by taking into account the total experimental conditions under which the phenomena appear, it is seen that, while both descriptions are necessary for complete elucidation of the objects, they are not contradictory. Bohr then called them complementary. But it is regrettable that he should

[1] "Discussion with Einstein" in *Albert Einstein, Philosopher-Scientist*, ed. by P. A. Schilpp (Evanston, The Library of Living Philosophers, 1949), p. 209.

repeatedly propose the application of a principle which, according to the usual standards of scientific exactitude, is exceedingly vague in its formulation.

Though Bohr stressed the importance of the principle in physics, he was equally emphatic regarding its fruitfulness in other scientific domains, especially biology and psychology. He often prefaces his excursions into other fields: "When searching for harmony in life one must never forget that in the drama of existence we are ourselves both actors and spectators." More explicitly, this thought suggests that our position in nature is peculiar insofar as our ways of knowing are partly active. Our interrelation and interaction with that which we seek to know means that we cannot help modifying it in the very process of coming to know it. This view of knowledge represents for Bohr the common element in various fields of inquiry. It implies to him that we may expect to find dilemmas similar to that in physics in other scientific fields and may expect that complementarity will apply in a similar fashion. The chief leg upon which the proposed analogy stands is the fact of *unpredictable* experimental interference with the course of an object's behavior.

The dilemma of duality which Bohr saw in biology was the age-old conflict between a mechanistic and a generally vitalistic [2] view of the organism. We suspect that, although Bohr uses the term "vitalism" to refer to one of the complementary views, he probably does not mean "vitalism" in the strict classical sense.[3] Instead it seems evident that he merely wishes to emphasize and contrast the phenomena of teleology and of mechanism. With respect to those of vitalism, or, as he sometimes calls it, finalism, he refers to "the self-stabilizing powers of the organism," "the domain of biology which is characterized by teleological arguments," [4] and "concepts directly referring to the integrity of the organism transcending the scope of these [physical and chemical] sciences." [5] These special characteristics of animate matter are often called emergent properties and refer, for example, to such phenomena as homeostasis which serve as marks of a teleological system.

The analogy, then, as Bohr delineates it, points to two mutually

[2] "Discussion with Einstein," p. 236.

[3] If he did mean "vitalism" literally, the analogy obviously would not apply, since vitalism postulates an ontological dualism between vital and non-vital processes and substances.

[4] *Atomic Theory and the Description of Nature* (Cambridge, University Press, 1934), p. 118.

[5] "Physical Science and Man's Position," in *Philosophy Today*, Vol. 1 (March, 1957), p. 67.

exclusive experimental and theoretical frameworks within which the organism is observed and described, i.e., the mechanistic or broadly physico-chemical and the organismic-holistic view. Any experimental arrangement allowing for the detailed physico-chemical analysis required for complete mechanistic determination of the organism would prohibit, as Bohr says, "the free display of life." For it is obvious that at the limit in such analysis the organism is necessarily—dead. On the other hand, observation of the whole intact organism, allowing the display of its (assumed) teleologically directed functions and its complex vital activities, prohibits simultaneous analysis along physico-chemical lines.[6]

Furthermore, while it seems evident that many of the basic life processes are governed by quantum laws, yet to define unambiguously these processes through experimental examination is again to destroy the organism's life. That is why Bohr can say "we are unable to define the state of a living being in terms of atomic measure,"[7] in spite of the fact that quantum regularities are apparently fundamental in the stability of the chromosomes, in mutations induced by radiation, and in the sensitivity of the perceptual organs. In other words, it would seem, according to Bohr, that although it is possible under very precise conditions to obtain detailed specification of quantum relations in biological systems, something is irretrievably lost in the process and that that something cannot in principle be retained for observation under such conditions. Thus, the definition in quantum physical terms of the organism's state can only be partial, incomplete without the complementary phenomena which will be preserved for observation under other conditions.

In this connection Bohr suggests that "life" may be a fundamental or elementary notion in biological science analogous to the "quantum of action" in physics. As we become more and more exacting in examination of animate matter we may find that life is a limiting factor which functions epistemologically somewhat like the quantum of action. That is, it may act as a kind of limit on the knowledge we may obtain of the objects conditioned by it. Because of its existence no one set of experimental conditions can yield the complete set of potential phenomena; i.e., both a detailed physico-chemical specification of the organism and an adequate description of its characteristically vital, teleological

[6] Actually Bohr's references to the analogy are rare, even though invariably emphatic. In addition to the previous citations see also *Atomic Theory and the Description of Nature*, pp. 20–23, 117–119.

[7] *Atomic Theory and the Description of Nature*, p. 22.

behavior cannot be obtained simultaneously. Only mutually exclusive experimental arrangements, yielding *prima facie* incompatible (i.e., interpreted as animate and inanimate) phenomena, can lead to a complete description of the object. Then, according to Bohr, this dilemma of duality can be resolved by the principle of complementarity just because the seeming contradiction arises from the fact of unpredictable experimental interference and the subsequent necessity of mutually exclusive arrangements for the complete determination of the object.

When Bohr extends the analogy to the psychological aspect of experience he suggests that a dilemma may lie in what he sees as the necessity for description in both physiological and "psychical" terms. With regard to quantum physics the epistemological problem in part arises from the fact that we must use the same basic stuff both as means of and as object of observation; in psychology we have at least what appears to be a very similar situation. When we study the elementary structure of matter, the measuring devices we use consist of precisely the same elementary units as those of the matter being studied. The resultant possibility of uncontrollable interaction and of the unpredictable occurrence of indivisible quantum processes ends in a blurring of the line we usually construct between object and observer.

A similar interaction may occur in the observation of introspective processes in psychology. For some purposes and according to some viewpoints introspection is one inevitable tool in the development of a scientific psychology. Certainly, from the common-sense standpoint, the attempt to understand better our own feelings and thoughts cannot even begin without the ability to think about our thoughts as they are being thought. But it is common knowledge that to try to follow consciously the stream of consciousness, or to observe coolly one's own emotional experience, may to a greater or lesser degree alter the course of that stream. One is left, then, without possible knowledge of what *might* have been, had the interference of observation not been imposed. Thus, as an analogy to the physical situation we have a system—the psychical experience—which in the very process of its observation is unpredictably altered.

We must note another related kind of experimental interference which leads into similar difficulties. Assuming or granting a correlation between purely psychological ("mental," conscious) experiences and some kind of physiological mechanism (neuromuscular states and processes), an attempt to determine clearly one correlate must alter the other so that both cannot be determined

fully under a single experimental condition. If an example is needed, it might be suggested that to subject an individual to the exhaustive examination of his brain and nervous system necessary to define their physical state must necessarily alter the contents of his consciousness, perhaps even to the point of obliterating any such contents. In terms, then, of this loose, general, and speculative use of the analogy it may be that consciousness is another limiting factor, allowing us to achieve knowledge in this area only in complementary bits and parts, achieved only under a complex of mutually exclusive conditions.

II

An analogy vaguely stated and left loose may have value in exploring general similarities in diverse fields, but a general principle, if it is to have concrete value in application, must be clearly formulated and capable of precise statement of its function. Bohr, however, does not provide a clear and concise statement of his principle; he refers instead to areas of its possible application and provides examples. A more explicit statement of it is needed if his analogies are to be evaluated.

The principle should function as a meta-linguistic rule, telling us how to use the language of classical physics unambiguously in the description of quantum phenomena. It requires that the total experimental conditions under which the phenomena are manifested be included in description of the object's state. Thus properties are not attributed to an object in the conventional sense but to the whole context of interaction within which they arise. We may represent the situation with the following schema. Suppose in an experimental set-up A, devised to test theory T in its application to some object X, data D are obtained from X, and the data, interpreted in theory T, lead us to conclude

$$X \text{ is } Y. \tag{1}$$

Suppose in another set-up A', which excludes the establishment of conditions A, and devised to test the application of theory T' to X, data D' are obtained from the same X,[8] which give, when interpreted by theory T',

$$X \text{ is } -Y. \tag{2}$$

D and D' are called, following Bohr, "complementary phenomena," for they constitute evidence or data which can be interpreted classi-

[8] We know it is the "same" object either (1) operationally, e.g., "Light is what we observe when we push this switch"; or (2) because the object is defined by other criteria, e.g., "Electrons have a charge of . . . etc."

cally only in terms of contradictory theories (wave and particle) and which were obtained under experimental conditions which cannot be applied simultaneously. Bohr says they "cannot be comprehended within a single picture, but must be regarded as *complementary* in the sense that only the totality of the phenomena exhausts the possible information about the objects."[9] But (1) and (2), as they stand, are contradictory descriptions. The meta-linguistic rule of complementarity says: the statements must be revised as follows in order to describe unambiguously the situation in classical terms:

X under conditions A is (or acts as if it were) Y; (1′)
X under conditions A' is (or acts as if it were) $-Y$. (2′)

(1′) and (2′) are complementary descriptions.

Contradiction arises only in the classical framework, but Bohr believes that examination of the presuppositions for knowledge in physics shows that we must operate in this framework at the point of experimental procedures. If this is the case, then, since the object and its properties cannot be defined except within and including the conditions of the experiment, the use of the classical representations cannot be escaped. This is essentially why some principle must be found to resolve the inconsistencies which arise in the attempt to apply the classical terminology to radiation effects.

Granting him this, we must suggest two cautions in the hope of avoiding misconceptions of the scope of the principle's application in physics and of avoiding hasty and improper application of it in other scientific and philosophical fields. We refer to two sets of conditions which, if met, would either suggest a resolution of the "dilemma" without appeal to the principle of complementarity, or would show that the dilemma need never have arisen.

First, with respect to physics, if it could be shown as a matter of empirical observation that wave phenomena are related as some function to particle phenomena and vice versa, then the two descriptions would be interchangeable and not complementary. Predictions on the basis of the observation of one set of phenomena could then be made and verified as to the values of the other set of phenomena. The two theories would be connected because $D = f(D')$, where D = wave phenomena and D' = particle phenomena. In that event, some true propositions in one theory would be associated with true propositions in the other, even though the language would be different. Also, neither theory containing an as-

[9] "Discussion with Einstein," p. 210.

sertion of this connection would be elegant because the permission to infer the particle-phenomenon from the wave-phenomenon would not be a theorem in either theory, but only an *ad hoc* rule. With respect to the *classical* wave and particle theories this sort of permission is not granted for those phenomena to which the Principle of Indeterminacy is relevant.

To generalize, then, any "dilemma of duality" would dissolve if it could be shown that one classical theory could make the same predictions as the other. (We shall attempt to show later that, unfortunately for the analogy, precisely this means of resolution of the "dilemma" does exist in biology and perhaps also in psychology.)

The second possibility for unification without complementarity would arise if the following could be shown. Moving outside the classical framework entirely we might employ some such concept as that of a "wave-packet which can interfere with itself," or a "wave-packet which can screw itself up into a point." We might speak of "wavicle" or "probability distribution of particle in a wave," or "for every particle an associated wave," etc., based on an encompassing *non*-classical theory (e.g., wave mechanics). Some physicists say that in this way the problem is entirely bypassed; for them there is no problem.

Although it is certainly recognized that these concepts are used—Bohr himself, of course, is a central figure in the development of wave-mechanics—yet, at least with respect to physics, this does not, according to Bohr, solve the epistemological problem. The reason is that in this case we are using the words "wave" and "particle" not at all in the classical sense; classical waves and particles certainly could not behave in such ways and accomplish such feats. It is perfectly all right, according to Bohr, to develop any kind of representations to serve as theoretical aids. However—and this is the stumbling block—at the point of experimental checking one must return to the classical framework, within which alone experimental procedures can operate. Evidently, experimental procedures do not reveal a "wavicle," but either wave-like behavior or particle-like behavior, *and not both at once.*

III

We are now in a position to consider in more detail Bohr's analogy in biology. In general terms, the biological debate referred to by Bohr revolves around the question whether in the analyzable "stuff," in its various relationships in the organism, we can ultimately hope to find all that is necessary for the complete elucidation of the organism, or whether we must look beyond this

to some other, perhaps incompatible, concepts or principles such as teleological order, "dynamic wholeness," or even some unanalyzable "spirit" substance.

Granting the conflict among the theories, the only relevant question here is whether the contrasting theories may properly be said to be complementary. Close examination reveals an inappropriateness in the application of the principle which may lead either to silly or to harmful effects.

As contrasted with contemporary physico-chemical analytical theory, organicism or holism seeks to attribute a causal efficacy to the functioning of the "whole."[10] This general concern often leads to some kind of teleological interpretation where, it is felt, certain "wholes" manifest some properties which cannot be fully accounted for on the basis of the physical sciences. These are the so-called "emergent" properties. Emergents, in contrast to "resultant" properties, cannot be predicted or deduced in advance of their appearance in *some* whole, even on the most complete understanding of the properties of the constituent parts in isolation. However, *after the emergents have appeared,* the physico-chemical elements upon which their appearance depends may be analyzed, and the order and interrelationships of these elements may be specified. Emergent properties are related in a unique way to the physico-chemical constituents of the whole. They are not related as a sum is related to its parts in their various commutative relations (e.g., the weights of a whole to the weight of the parts), nor as a property of a structure of parts in a fixed, non-commutative relation according to an *a priori* plan (e.g., the area of a square as related to the length of its sides). Still, emergents may be specified with respect to the parts of the whole without prejudice to their special and unpredictable qualities.

It may be possible to discover an empirical law which will relate the total (experienced) property to the structure of the parts. That is, the complex set of conditions may be found which, when they are present in a particular, specifiable arrangement, will give rise to the experienced property. Symbolically,

$$\text{E.Q.} \equiv f(P_1 \ldots P_n)$$

where "$\equiv$" means "operationally equivalent," "E.Q." means "emergent quality," and "$P_1 \ldots P_n$" represent the parts in their actual relations in the whole.

[10] Though such concern may sometimes lead to a radical subjective teleology and to vitalism, it need not necessarily. We do not discuss pure vitalism, for as a scientific hypothesis it has been shown to be particularly unfruitful and few responsible biologists today embrace this view.

Now life is a property marked by, e.g., homeostasis. The chief characterizing fact about homeostasis and other vital phenomena is that they can be roughly identical under *different* conditions, e.g.,

$$\text{E.Q.} \equiv f(P_1P_2P_3 \ldots P_n) \text{ or } f(P'_1P'_2P_3 \ldots P_n),$$

(the emergent in this case being homeostasis). That is perhaps why there is a considerable temptation to say that the parts function *so as to* maintain homeostasis even in the face of a considerable range of changes in these supporting parts. However, all that need be said is that the emergent quality will result under the following conditions.[11] It will appear if $(P_1P_2P_3 \ldots P_n)$ or $(P'_1P'_2P_3 \ldots P_n)$, but will disappear if $(P'_1P_2P_3 \ldots P_n)$ or $(P_1P'_2P_3 \ldots P_n)$; but there is no one way or reversible causal relation $P'_1 \rightleftarrows P'_2$ but only a causal relation $(P'_1P_2P_3 \ldots P_n) \rightarrow (P'_1P'_2P_3 \ldots P_n)$ or $(P_1P'_2P_3 \ldots P_n) \rightarrow (P'_1P'_2P_3 \ldots P_n)$. Here, clearly it is neither necessary nor possible to hypostatize the emergent quality as an "added agent" permitting $P'_1 \rightleftarrows P'_2$ *in vivo* but not *in vitro*, because $P_1 \ldots P_n$ in their particular interrelation condition this change. In other words, one of the *effects* of the complex, uniquely reciprocal relations among $P_1 \ldots P_n$ will be the endurance of the emergent quality.

If, as we have attempted to show, the special characteristics claimed by organismic-holists for the whole—emergent qualities, teleological functions, etc.—are, at the least, a function of physico-chemical elements (and possibly even reducible to explanation in terms of these sciences) then immediately the complementarity argument breaks down.[12] Instead, a vertical structure of explanation with holistic and physico-chemical methods occupying different (but not incompatible) levels of explanation suggests itself. Organismic experimental technique, by allowing the organism to remain essentially intact and whole to the degree necessary for the emergent quality to be manifest and by carefully varying internal and external conditions, delineates certain major groups of phenomena and broad patterns of behavior.[13]

However, the phenomena obtained through these methods cannot be interpreted as irreducible representations of the manifesta-

[11] The analysis here is based upon E. Nagel, "Teleological Explanation and Teleological Systems," in *Vision and Action*, ed. by Sidney Ratner (New Brunswick, Rutgers University Press, 1953), pp. 192–221.

[12] As we have shown above complementarity can no longer be applicable if it is found through empirical observation of the phenomena that one of the competing theories is actually a function of the other.

[13] In this procedure the provisional assumption that all the underlying (mechanistic) changes are goal-directed with respect to the emergent quality may, of course, prove helpful.

tion of one set of the object's potentials. It cannot be asserted that these representations cannot be translated into terms of the phenomena obtained through the other approach, i.e., physico-chemical analytical procedures. In fact, it is these very procedures which are demanded as the next logical step after organismic procedures have laid out the broader patterns and thereby provided *clues* as to the general kinds of constituents to be expected in the systems examined. As physical analysis proceeds, i.e., dissection and assaying of a teleological system, we reduce to physico-chemical status, *in vitro,* all the constituent elements of the parts, which in their relation (now destroyed, yet thereby discovered) condition the appearance of the emergent quality.

Thus, the teleological framework adopted in the organismic-holistic approach may serve as a sort of first-level analysis, fruitful in pointing the direction to be taken in a more detailed scrutiny of underlying conditions. This relation of the two theoretical frameworks in biology is not comparable to the relation in physics assumed by Bohr to be analogous to it. There, no translation is possible between the two classical theories; they are mutually exclusive. Here, central elements of one theory are shown to be a function of the other. Here, the two theories do not stand side by side, incompatible without the concept of complementarity, but instead represent two stages or levels of inquiry whose results do not conflict with one another.

Obviously also from the standpoint of experimental procedures and observational devices the analogy appears just as tenuous. In physics the same general type of precise mathematical devices are combined in both cases with experimental procedures which are methodologically parallel. These yield phenomena described in terms which in both cases (wave and particle description) are quantitatively exact to roughly the same degree. The methods of an organismic-holist operating under the assumption of teleological order differ markedly from those of the bio-chemist dealing with the action of some enzyme within the organism. The methodologies, like the results, do not represent the same order of things and thereby fail to fit the analogy.

IV

It is noteworthy that the claim that complementarity is useful in psychology has been made, so far as we are aware, only by physicists and theologians. Psychologists seem never to allude to the concept that is supposed to save them from dilemmas and embarrassments—some of which we suspect they have never faced or felt. Therefore, our concrete question is: Are there any concep-

tual and/or experimental dilemmas in psychology to which the principle of complementarity may apply in such a way as to eliminate or at least illuminate them?

Bohr's words in some places suggest an opposition between what he terms a "psychological point of view" and a purely "physical view." The "physical" represents a "pure scientific" approach to human behavior with a view to describing and explaining it (*beschreibende Psychologie*), and the "psychological" represents a more direct, intuitive approach with a view to "understanding" (*verstehende Psychologie*). Bohr says that in the study of certain kinds of "innate and conditioned behavior" it is necessary to use not only the full resources of physico-chemical analysis, but also concepts "directly referring to the integrity of the organism transcending the scope of these sciences."[14] Bohr seems to suggest, then, that in the study of human behavior we must use not only the tools of the physical sciences (this would no doubt include all the "scientific," experimental, behavioristic, physiological schools of psychology), but also that other category of approach which is based upon an intuitive grasp which all of us have to a greater or less extent as a natural consequence of our own active, intelligent human condition. And these two approaches are to be understood as complementary.[15]

However, even if this second category be admitted as psychology proper, the analogy is extremely vague and tenuous. For when the "understanding" approach is set against the "scientific" approach we see that the methodological considerations and experimental techniques (if we can say that the intuitive approach uses experimental techniques at all) are so diverse that there can be no question of comparing or relating the results quantitatively. There is no common conception of what the object is, what are the state variables which cannot all be accurately accounted for in one or the other framework; in short, these two approaches in the field of human behavior do not operate on the same level with each other, or within a broader framework having many conceptual elements in common such as is the case with the wave-particle dilemma *within* classical physics.

As already noted, Bohr reminds us several times of what he calls "the old truth that we are both actors and spectators in the great drama of existence"; and this usually is an explicit justification for his transportation of complementarity out of physics. In psychology, a case could be made for the actor-spectator duality

[14] "Physical Science and Man's Position," p. 67. See also *Atomic Theory and the Description of Nature*, and "Discussion with Einstein."

[15] "Physical Science and Man's Position," p. 67.

as referring to the distinction between reason-giving (actor-category) and cause-finding (spectator-category). That is, considerable disagreement might arise regarding some behavior of the organism depending upon which category or point of view comprises the framework of explanation. The actor, in explaining his act, always adduces reasons, and, if pressed, he will try to show that these are valid and well-grounded. The spectator seeks to find factors correlated with initiation of the act, either immediately (such as neuro-muscular or endocrine states and processes), or remotely and through a long causal series, culminating in the act (such as events and reactions in the individual's history brought to bear upon the present situation through laws of association, etc.). There is a kind of mutual exclusion in these two categories; certainly both could never be applied at once by the same person. However, the analogy does not hold up, for it is easy to see that in a debate of this kind the categories make reference to and disagree over interpretation of one and the same phenomenal act—not to different acts each of which requires one or the other category but not both for its explanation. Also, both decision (actor-analogue-of-prediction) and prediction (spectator-category) with respect to the act must lead to the same objective result or else the prediction is wrong. The case is precisely opposite in physics (where complementarity applies), for there the two contexts of prediction must lead to different, and *prima facie* incompatible, results.

It would seem that Bohr intends his analogy to apply to two *kinds* of psychology, *verstehende* and *beschreibende*, and we have attempted to show how this application breaks down. Scientific psychology, particularly as developed in America, is the descendant of the biological sciences rather than of philosophy and humanistic study. It has taken the attitude of spectator of human behavior, and, like any other scientific spectator, is allowed no active participation except experimental control of a few variables. The conflict which Bohr intended to resolve with his principle, therefore, is not one within psychology as an objective, experimental science.

If, nevertheless, an attempt is made to apply complementarity *within* psychology as a *science* the situation probably reduces to that of biology. Several areas within scientific psychology might superficially appear to be possibilities for the application of complementarity—e.g., the different perspectives arising from different experimental placements of the subject-object dividing line; the distinction between a molar and a molecular framework of reference both in behavioristic psychology and in introspective psychology; or the possible opposition between introspective and

behavioristic approaches themselves. However, close examination reveals that in each case, as in biology, either we have but one set of phenomena interpreted in two ways; or the allegedly opposed frameworks are not even considering the same object, so that though there are indeed two sets of phenomena they are manifestations of two or more objects; or the two frameworks may be considered as two levels of explanation with one a function of the other and with translation from one to the other permissible.

We should be particularly cautious in expecting complementarity to be clearly applicable except in a science in which the theoretical structure is so rigorous and explicit that there can be no debate as to what belongs to a theory and what is only collateral to it. It is not perfectly clear that psychology has yet achieved a sufficiently high degree of internal theoretical integrity to permit the unequivocal application of a meta-linguistic rule of this kind.

V

Bohr's concept of complementarity, in its vague general formulation and its loosely suggested analogies, illustrates a general philosophical outlook which has been clearly delineated since Whitehead and Mead. Moreover, in a non-technical, common-sense way the concept has probably been always with us, representing the insight, profound in its simplicity, that, in the search for understanding, to peer narrowly out at things, fettered to a single-sided point of view, may yield nothing better than half-truths; that a too-meticulous dislike for apparent contradictions may cast a veil, in the name of theoretical tidiness, over the sometimes many-sidedness of things; that for all that can be done, some questions can be fully answered only by finding a way to countenance two superficially conflicting points of view which, taken together, more nearly approximate to truth than can either one, no matter how cherished, do alone.

Experience presents countless examples of situations to which a certain sort of complementarity is, perforce, applied. Toleration between person and person would not be possible without an attitude that permits incompatible, even explicitly conflicting, views to exist side by side in recognition of the diversity of possible meaningful frameworks of reference. No doubt, even within individual experience the incredible variety of impression, of response, of commitment, of hopes, cannot always be entertained without some sense of the contradiction which arises in an attempt to encompass all within a single simple frame of interpretation or structure of ideals.

In view of these considerations the concept seems perhaps not very startling. The concept of complementarity tends to add weight to objective relativism, which claims that we know an object only within various contexts; indeed, we fully know it only as it appears in the totality of its relationships.[16] In short, we cannot know an object in complete isolation either from ourselves or from other objects. It is the perceptual context, that is, the object in the context of our own observation of it, which is frequently overlooked as a context, or taken for granted. Objective relativism, like Bohr, emphasizes that this is indeed a context, with the result that what we know is not a something having some transcendent ontological status in and of itself, but only a part of a totality of objective appearances defined by their relations to one another, that is to say, defined in various contexts.

Though on the one hand Bohr's reflections on complementarity, and the precise physical considerations from which they grew, explicitly illustrate and support objective relativism or contextualism, at the same time and for this very reason it is seen that the principle of complementarity is not the novel and perhaps revolutionary concept which some of its supporters would like to claim.

Further, in a primary science as highly abstract and integrated as physics, careful formulation can make the precise and legitimate application of the principle perhaps reasonably clear. However, in the secondary sciences where a comparable degree of precision and elegance is not yet present, the principle can find only vague and possibly misleading application. A premature application of the principle to the wrong sorts of elements in a science (to overall theoretical conflicts not grounded in an experimental situation comparable to that in physics) may very well prejudice the kind of answers found, or, even worse, it may prevent the asking of important questions or divert the open-minded search for knowledge, under the delusion that complementarity has solved the problems.

The dangers to scientific endeavor of molding thought and research to a preconceived notion based on a loose analogy with

[16] The everyday point of view—the "classical"—assumes that, given a knowledge of all the relevant contexts within which an object may be defined and given the specification of one context at a certain time, the object could then be specified precisely with respect to its other contexts and for future times. In this way a complete knowledge of the object could be gained. The new situation in physical theory, especially as formulated in the Indeterminacy Principle and more explicitly by Bohr, of course shows it is just this necessity for contextual definition of objects which makes completely accurate specification of all manifestations of the object's potential behavior for any one time impossible.

quantum physics, or anything else, are obvious. Wishful thinking bolstered by a certain naive trust in the universal applicability of the discoveries of modern physics notwithstanding, there is no justification for rash and eager application of this principle, in the name of modern physics, to the quarrels of "religion and science,"[17] to international misunderstanding,[18] or to the conflicts between the eastern and western world-views.[19]

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OF RECENT ORIGIN

THE writer proposes a philosophic reconciliation of the antithetical properties of the ether.

On opening a bottle of cold club soda and adding a few grains of graphite particles from a crushed pencil lead, then recapping, the writer has been able to make the following observations:

1. Carbon dioxide bubbles instantly appear on the graphite particles which act as irritants inside the solution.

2. These bubbles animate each grain with buoyancy.

3. On reaching the surface the bubble bursts and the unsupported particle sinks inertly to the bottom where new bubbles appear and repeat the cycle.

4. After allowing the solution to subside, then by partially lifting an edge of the crown cap the writer was able to reduce the gas pressure which would inhibit the formation of new bubbles in a Pascal doll effect.

5. Now the less turbulent particles were no longer able to break through the surface but were seen to dash against the inner energy shell of surface tension and bounce back as though from a steel plate. The top of the surface remained quite placid giving no hint of the activity below.

6. Further, the small bubbles themselves were now too weak to penetrate the energy shell of surface tension and were bursting under water, decaying immediately back into solution.

7. At night when the temperature of the water dropped, no more carbon dioxide came out of solution and all activity ceased. In the morning, warmed by the sun, carbon dioxide spontaneously

[17] As pointed out by Peter Alexander, "Complementary Descriptions," *Mind*, n.s., Vol. LXV (1956), pp. 145–165.

[18] "Physical Science and Man's Position," p. 69.

[19] Wolfgang Pauli, "Die philosophische Bedeutung der Idee der Komplementärität," *Experientia*, Vol. VI (1950), pp. 72–75 (Summary in English).

appeared on the irritant particles and the rhythmic rise and fall resumed. There was no activity on cold days. There was some activity on warm nights.

8. About noon on hot days the gas pressure became prohibitive and all activity ceased; however, at night when the system cooled carbon dioxide was reaccepted into solution and the pressure slackened in preparation for the next day's activity. However, in time the Pascal effect did become prohibitory. Then, two bottles were joined by a glass tube, and one bottle was buried 30″ below ground at a constant 60° F. The other bottle was exposed to the sun with temperatures from 80° daylight to 40° at night. Now the cool lower bottle accepted the carbon dioxide during the heat of the day and returned it to the upper bottle during the night. In this way the Pascal pressure effect was avoided. But now the system contained carbon dioxide alternating between states of discrete bubbles and continuous entropic disorder, each state being of recent origin. As a result, the writer is able to say he has had bottles working since March 18, 1957, without noticeable reduction in activity.

And the writer cannot suggest why there should ever be a total reduction in activity because once a given bubble had endured the attrition of friction it burst and decayed, never again to return as the same entity embracing the same atomic components. But the fate of a given bubble is meaningless in a system designed to produce new bubbles in infinitely expendable generations, each bubble being individually endowed with all the vitality and vigor of recent origin. As to the carbon particles themselves, once the bottle is glass-sealed they can neither escape, nor disappear, nor combine. They can only remain as irritants. Thus the writer is able to show that although friction cannot be eliminated from a system with moving parts, it can be reduced to a factor approaching the value of zero by automatically replacing the worn parts with new ones, all of absolute expendability.

Science finally abandoned the ether because it had acquired so many properties, some of them in flat self-contradiction, that it became a mathematical burden, and because it became suspiciously docile, doing everything necessary to be done. However, the writer proposes a more conciliatory review of the matter by regarding the ether as a solvent containing in solution all the energy fields which contribute their varied and often antithetical properties. In this way the classical purity of the ether itself can be restored without sacrificing any of its properties. For example, let us prepare a solution of salt saturated in water. Since the solution cannot make room for another single ion it seems reasonable to assume the water presents to an outside ion an imcompressible

ionic field with the resilience of billard balls in continuous contact. And on a relative basis this field may be solid enough to a photon to provide the medium essential for the transmission of light at high velocities. But, saturated as the water may be with small contacting fields of ions, it can readily accept an additional solute as, say, a drop of blue ink. Now we have an incompressible field of ions, a compressible field of blue dye, and a "solid state," all in one solvent.

And the capacity of the solvent is by no means exhausted for the accommodation of even more fields, each contributing its own individual properties. For example, when the writer presses a switch in his room he generates a field of light. From a flower vase comes a field of scent, from the radiator a thermal field, from the radio a field of sound, and from the pan of water a field of humidity. And, of course, there were gravitational and air pressure fields to begin with—a veritable orchestra of energy fields.

An orchestra of energy fields. There is indeed an analogy between a group of energies dissolved in an ether, and instruments blended into an orchestra. Even if we intermingle the instruments throughout the orchestra, they continue to generate their own characteristic fields by responding to identical scores with the same instruments, or maintain similar periods of inactivity. Thus, when we hear silvery blue notes we instantly recognize the presence of a field of flutes, as sparks from a comb show the presence of a static field; and a dry oriental timbre indicates a field of oboes as surely as iron filings reveal a magnetic field. Further, when the prearranged quota of sixteen violin chairs is filled, an additional violinist is quite unable to find an available seat, and the orchestra may be regarded as in a state of violin field saturation. But cymbalists and harpists may readily find chairs.

Tonight a photon will come to rest here with all the brilliance of recent origin after a flight of a million years. The earth has aged and the stars have aged. Why not the photon?

Let us contemplate a photon being emitted at this instant from our sun. There is reason to believe it is emitted as a discretely ponderable packet by an excited electron; however, it blends into a felicitous medium with such velocity that in one second it achieves the proportions of an attenuated and subvisible shell of energy with a radius of 186,000 miles. Now let us freeze the shell momentarily and return to the original scene of birth. A second photon is emitted and it also expands into a shell concentrically within the first shell; then a third shell, and so on. Now let us unfreeze the shells and take up an observer's position on earth. To us down

here these shells arrive in periodic intervals of energy, so that we find it only reasonable to assume that although they were emitted as discrete packets these photons are undeniably reaching us as waves of energy.

Now let us take up an observer's position next to an "orbital" electron. When the first layer of energy arrives the electron absorbs a discrete portion of energy at the point of contact, like a rock in the path of waves. As more layers arrive the electron ingests more and more energy. Then, after it has accumulated a given quantum of energy, the excited electron delivers it as a newly born photon, which in turn repeats the original process by expanding with fleeting brevity into a subvisible and attenuating shell of energy. Only now the new photon consists of tiny parts of an infinite number of ancestral photons. Therefore, what was once a particle is now a wave, and, since it could hardly be gathered up and compressed back into its original particle state, nature recovers it piecemeal and restores it inextricably intermingled with all other photons. Thus it appears that the difference between states of energy and matter lies in a degree of relative attenuation with no clear-cut line of demarcation.

In this way the interstellar transmission of energy is accomplished and matter is intermingled on a level approaching infinity, and at the same time the universe maintains the ageless vitality of recent origin.

However, when light strikes a metal, why do some of the electrons stream into a current? Since electrons are identical, why do not all that are exposed on the surface respond *en masse* to identical light energy? Again, why do they respond with energies independent of the light intensity, but numerically proportional to it? And why do their energies depend on the frequency of the light? Besides, what of the time required to absorb this energy of emission? Precise measurements indicate that if such a lag exists it is less than 3×10^{-9} seconds. This seems to preclude the energy absorbtion interval required by electrons in the preceding paragraph.

The writer suggests an analogous explanation to these questions: First let an apple orchard represent a sheet of zinc, and each tree be an atom, and each apple be an electron. Then let a wind be light, descending from the mountains, undulating in waves across a wheat field and sweeping through the orchard. Of course some apples will fall. But which ones? Only the ripe ones; the small green apples will cling to the tree. Now if electrons be allowed to ripen energy-wise, only the ripe electrons will fall when struck

by the light wind. And since a ripe apple is detached with the same finality whether the wind be mild or strong, the energy of the apple is independent of the wind intensity. However, a strong wind will obviously dislodge more apples than a weak wind. On the other hand, the kinetic energies of wind-borne apples (evaporating dew would have made a better analogy) will depend on the frequency of the waves of wind. As for time lag, none is required. Ripe apples fall on contact. As a matter of fact some apples will fall in a calm day without contact. This may be regarded as thermionic emission providing the electron-apples lying around loose between the atom-trees. And obviously the fall must be complete to the ground. Half falls could hardly be permitted.

Thus, when we examine the light wind, some will say it is wave-like in character. Others will concentrate on the embedded apple-electrons and pronounce it a shower of particles discrete in character. It should be suggested here that these particles evaporate, or are consumed in work, or decay like apples returning their energies into the wind or to the atom-tree for release to the new electrons which are eternally ripening, in a process of recent origins.

Now, returning to the photon, since ponderable matter could never attain the velocity of light, it was essential that the photon evaporate into waves of energy prerequisite to flight imparting its energy to the ether for transmission equally and undiminished in all directions; and, since all these fields in solution presuppose a liquid, the ether emerges once more as a factor that must be recognized. And the writer predicts the ether will prove out mathematically as a solvent of energies containing all the component fields which endow it with apparently irreconcilable properties, and from which the entire universe, including life itself, emerges with individually finite age of origin.

It was necessary for the writer to see significance in the experiment because it showed clearly why friction has not long ago reduced this whirling universe into a wave of thermal energy; and he suggests that a state of maximum entropy would have indeed prevailed by now were it not for the principles of absolute expendability, and of recent origin. In other words, if all the stars in the sky disintegrated into thermal energy and dissolved back into the ether, a cold, super-saturated condition would result, forcing fresh hot stars out of solution in an endless procession of new creations. And this cycle will forever unravel in a sustained obdurate and invariant sequence of recent origins, eternally precluding a state of maximum entropy.

The photon could never get here any other way.

The writer found himself bemused on occasion by a system in which a bubble appeared on an inert body and animated it into a pattern of behavior that parallels life itself. Each bubble toils upwards, struggling for energy, colliding hostilely with other bubbles, pressing higher and higher, impelled by the same imperious urgency that drives all life toward a pre-imposed goal; yet, on reaching the goal, it merely bursts and releases the burden from its decaying grasp with the wry expendability of a wave by night. Although the laws of probability preclude the return of any given bubble, it is quite obvious the bubble must and will appear again, refreshed and renewed, as an integral part of every bubble that ever preceded it, or will ever follow out of solution.

Life begets life. And life resolutely destroys life. And life does both with a mother-wit from which all higher intelligence flows. An amoeba divides and ignorantly attacks itself, fish stalks fish, ant, ant, and man, man. There are at this very instant, in the savage depths of our oceans and jungles, protagonists behind cold masks enacting scenes of violence with such frenzied incompatibility as to transcend all levels of understanding and morality. Here is no compassion, nor escape, nor relief from pain, but the confusion of predestined antagonists who must first overwhelm the other, and then stonily unbeget him. Yet, after aeons of destruction of life by life, there is more life on the earth today than ever before.

Therefore they are only half right who regard the urge to beget as the prime force in life. The urge to unbeget is equally vital and must be recognized. Perhaps we are eternally denied the solution to the mystery of why we are here. But this much at least is now clear: Nature imposes three commands on every known form of life: You shall beget. You shall unbeget. And you shall rest. This is invariant. Beyond this there is nothing. And no matter how deviously civilized man has arranged to delegate the immediate act of subduing his prey, the ultimate act of unbegetting is inescapably his and can never be delegated. And the writer is impressed by the rhythmic compulsion with which all life responds to this paradox, for which the lofty ethics and philosophies of civilizations are often permitted to collapse and crumble in dusty neglect.

But the stars lie fair on the surface of the paradox, as on all paradox, for they see neither good nor evil, causality nor chance, mechanism nor vitalism, free will nor determinism. They see life, only life, an unfolding luminous and thriving flame, devouring itself.

Thus, to the poet who uttered in dark beauty the lament of

all humanity, "Where are the snows of yesteryear?," the experiment replies: Here, then, are the snows of yesteryear, falling in flakes, recurring soft and mild. And the same rains fall, and toss in oceans, and the same dew forms, and grass and amoeba and man.

A traveler appears, and is troubled, and whispers deep within himself: "How fitfully known is this scene which I behold for the first time." The experiment tells he has indeed seen it before, and it shall come to pass that he will behold it through many eyes in the future as he returns again and again to the constant scene. And he will mysteriously appear in the cycle of life and death "as part of every bubble that ever preceded or would follow." And he, like the universe itself, will be forever renewed in the fair and clear and bright miracle of recent origin.

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BOOK REVIEWS

La Révélation d'Hermès Trismégiste. III. *Les doctrines de l'âme.* IV. *Le Dieu inconnu et la Gnose.* A. J. Festugière, O. P. Paris: Librairie Lecoffre, J. Gabalda et Cie., 1953–54. xiv, 314; xi, 315 pp. (Etudes bibliques.) 2000 fr. each volume.

Father Festugière has been widely known among classical scholars for his numerous books and articles, some of them in English, which deal with the history of ancient religious thought, or more specifically with such philosophers as Socrates, Plato, and Epicurus. The work on the Revelation of Hermes Trismegistus, now complete in four volumes, represents Father Festugière's major effort so far. As an interpretative study, it is in a way the companion piece to the recent critical edition of the *Corpus Hermeticum* and other Hermetic writings by Professor A. D. Nock, to which Father Festugière contributed the French translation and some texts (4 vols., Paris, 1945–54). Yet the work, though perhaps initially intended as a monograph on the Hermetic writings, has turned out to be much more than that; it is a series of broadly conceived studies which cover in a wide range many important aspects of ancient religious and philosophical thought, for which the Hermetic literature merely serves as a starting point and central focus. The Hermetic writings, current under the name of the Egyptian god, Hermes Trismegistus, were actually written in Greek in Alexandria during the first two centuries of our era, and the extant *Corpus* is probably a Byzantine selection from a larger literature

of which at least some additional fragments have been preserved through other sources. This literature, though often obscure, sometimes trivial, and hardly consistent in its thought, as Father Festugière frankly admits, acquires its significance for the historian just because it is a typical, not an original, expression of many religious and philosophical ideas current in later antiquity. For the wealth of its information and of its suggestions, Father Festugière's work is an indispensable source of reference for any student of late ancient philosophy and religion, whether or not he is interested in the Hermetic writings as such, or happens to agree with some of the more general views expressed by the author.

The first volume (*L'astrologie et les sciences occultes*, 1942; 2nd ed., 1950) attempts to prove that astrology, alchemy, magic, and the other occult sciences became firmly established in Hellenistic thought, and were linked from the beginning with the "Hermetic" tradition. This discussion is supplemented by an important appendix on Arabic Hermetic literature, contributed by Professor Louis Massignon (pp. 384–400). Without pretending to have an expert opinion on this intricate subject, I should be inclined to say that Father Festugière is making a strong case for the early appearance of the occult sciences, and especially of alchemy, among the Greeks. I am not convinced by his arguments that the link between "Hermetism" and the occult sciences, characteristic of the medieval Arabic tradition, is itself of Greek origin. The number of Greek astrological and other occult writings which go under the name of Hermes is small; we do not know the precise date of these writings, or of their attribution to Hermes; nor does their transmission suggest any link with the *Corpus Hermeticum*, or with the specific environment in which the latter originated. Father Festugière makes much of the fact that a Hermetic text published by Gundel in 1936 contains astrological notions which go back to early Hellenistic times. Yet this text is preserved in a Latin manuscript of the fifteenth century. It remains still possible that other elements of this treatise, and the text as a whole, or its attribution to Hermes, are of much later, and perhaps even of Arabic origin. The term Hermetism, as so many similar terms, tends to become a rather vague label for a variety of loosely associated facts, as Father Festugière himself seems to realize at times. The first volume also contains a remarkable introduction in which the religious climate of late pagan antiquity is suggestively described (pp. 1–88), and an important study on the literary genera found in the *Corpus Hermeticum* and in other religious writings of the period (pp. 309–354).

The second volume (*Le dieu cosmique*, 1949) describes cosmic religion, that is, the religious worship and theology that have the sky and the stars for their object, as a pervasive trend in ancient thought. Whereas the Hermetic Corpus in which this theology is found as one among several elements is treated in the introduction, the major part of the volume traces the history of this notion from Xenophon and Plato through the early Academy, Aristotle, the Stoics, Cicero, and other eclectic sources down to Philo. The author rightly stresses that this cosmic religion supplied a popular substitute when the traditional Greek religion declined along with the city states in which it had flourished.

The third volume discusses the Hermetic teachings concerning the soul under the following headings: celestial origin of the soul; its fall; its choice of life; eschatology. In each case, the Hermetic teachings are illustrated through parallel passages drawn from middle platonic, neoplatonic, and gnostic sources. The volume contains two substantial appendices: a translation and commentary of Jamblichus' treatise on the soul, as recomposed from the fragments in Stobaeus (pp. 177–264), and of Porphyry's treatise to Gauros on the manner in which the embryo receives the soul (pp. 265–302).

The fourth volume deals with the "mystical" or "gnostic" element in the Hermetic writings, and with their sources. Aside from the "optimistic" element represented by the belief in an immanent, "cosmic" god, there is, according to Father Festugière, in the Hermetic writings as well as in the remaining literature of late antiquity, a "pessimistic" element based on the belief in an unknown or transcendent god who is completely detached from the visible world, and who can be grasped only by a higher, mystical or gnostic, form of knowledge. Within this mystic knowledge, Father Festugière distinguishes two kinds: "mysticism by extraversion" where the human self loses itself in God, and "mysticism by introversion" where God invades the human self (pp. IX-X). Father Festugière attempts to show in the first part of this volume that the notion of the unknown and transcendent God, as it appears in the Hermetic writings, is rooted in the Platonic and Pythagorean tradition. He then discusses the two forms of mystical knowledge in the Hermetic writings, emphasizing the notion of Aion, and the Hermetic doctrine of regeneration according to which a new man is created in us by divine influence. There are two appendices dealing with neoplatonic parallels, and an important addendum (pp. 307–315) which deals with the testimonies on Plato's lecture on the good.

I do not feel qualified to give an adequate description or critical discussion of this work in all its details, nor would it be possible to do so in a short review. Much of the value of the work consists in its rich philological apparatus which is always useful, learned, and suggestive, though not always convincing, or relevant to the main context: comparisons between a variety of passages, careful translations, discussions of many passages with reference to their text, grammatical structure, meaning, and terminology. There are interesting digressions on the history of important concepts, e.g., of that of existence (*hyparxis*, IV, 11–12), showing that it is well grounded in Greek philosophical thought (and hence not of Biblical or medieval origin). The structure of the work is based on some modern notions such as pessimism or optimism, introversion or extraversion, which are a bit schematic and artificial, as Father Festugière occasionally admits, and which make his picture appear at times almost as fuzzy and inconsistent as the Hermetic writings. One wonders whether cosmic religion, or mysticism by extraversion, are meant as real historical entities, or merely as facets of a more complex world view. And there remains something of a gap between the detailed analyses of passages and ideas, and the broad constructive schemes into which they are purportedly fitted. The author is at his best when he discusses literary forms, or gives psychological descriptions of the feelings and attitudes that seem to underlie the often obscure words of his sources, and to lend some human meaning to their strange abstractions. Those passages in the work display the qualities of a subtle, sensitive, and broad-minded writer and thinker.

It would be unfair to judge the work as if it intended to give a complete picture of late ancient thought and religion. The author is interested in pagan religious thought, and in those aspects of ancient philosophy that are connected with it. He omits all other aspects of Greek philosophy, and he does not consider the sciences as such. He treats Plato, Aristotle, and the Stoics only insofar as they influenced later religious thought; and, in the case of Plato, his interpretation is colored by the tendency to view him as a religious thinker, and to find in him some of the notions popular with later Platonists. He rarely speaks of Plotinus, because he is original and not typical (a curious and even dangerous criterion in the history of thought), and hardly at all of early Christianity or of the Gnostics; yet his material more than once throws an interesting light on some aspects of Neoplatonism, Gnosticism, and Christianity.

The most important merit of the book, in this reviewer's opinion,

lies in its general approach, and in some of its general historical ideas and implications which are to some extent well documented, and which will have to be considered and carefully examined, if not accepted. The importance of these ideas is the greater because they run counter to some tendencies prevailing in recent historical scholarship. Father Festugière maintains that the irrationalist and religious temper of much late ancient thought is not due to oriental influences, but to a kind of organic change which occurred within the Greek tradition, and which is traceable in all its elements to Greek sources (only once does he admit a novel element for which he cannot account in the Greek tradition, IV, 224). The *Corpus Hermeticum* thus comes to appear as a genuine document of ancient Greek thought, linked in many tangible and intangible ways to the preceding philosophical and religious tradition. It follows by implication that the role of Platonism and Stoicism in late ancient thought was greater, that of the mature Aristotle and of the exact sciences correspondingly smaller than some scholars have been willing to admit; that Greek philosophical and religious thought had a stronger impact on early Christian thought than it has been fashionable to say in recent years; and more generally, that ancient philosophy comprises besides its logical and scientific elements a genuinely religious component, whereas ancient religious beliefs and practices were to some extent nourished by philosophical ideas. In describing these ideas, Father Festugière shows an admirable degree of tolerance and understanding which many lay historians might do well to imitate, as they might take for their motto the words with which he concludes his last volume (IV, 267): "L'historien ne sait que ce qu'on lui dit. Il ne pénètre pas le secret des coeurs."

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Heidegger: Das Gefüge seines Denkens. PETER FÜRSTENAU. Frankfurt am Main: Vittorio Klostermann [1958]. 185 pp. (Philosophische Abhandlungen, Band XVI.) Cloth, DM 14.50; paper, DM 12.50.

This book makes a serious attempt to analyze the structure of Heidegger's thought. Although the author places Heidegger's *Sein und Zeit* in the center of his discussion, the development of Heidegger from 1927 to the present is indicated. The aim of the author is to introduce the philosophy of Martin Heidegger.

If we accept Heidegger's thesis that the ontological dimension of man points to man's most meaningful aspect because it says that

man in his uniqueness reveals the human dimension to himself, we begin our discussion with two words, man is. The noun, man, in its relation to the verb, is, reveals its dimension to be an ontogical one. The ontological dimension attempts to measure the meaning and uniqueness of man but only in an ontological way. The verb, is, refers to the dimension of the noun, man, and, refers to nothing beyond the noun. Implied in the structure of thought is the statement: man is man. Heidegger is concerned with the dimension of man ontologically and maintains that man becomes conscious of his being as Being insofar as man's being in the world reveals to him something about his dimension and uniqueness. In the religious framework it was man's relation to God or his separation from God which revealed the human dimension. The Heideggerian position thus states that being has no dimension beyond the ontological. Heidegger's statement about existence is that Man is revealed through man. The ontological is the primal structure of man and for Heidegger constitutes the total dimension of man.

To equate man exclusively with his ontological structure is incomplete. The incompleteness of this position lies in the fact that man ontologically revealed and defined can be conscious of this state of being only insofar as the state of being realizes itself as being something other than the practical states of being such as the political, social, and economic. These forms are for the ontological, the "other." These "other" forms of being are historical and describe man's doings, creations, and achievements. These are descriptive items or adjectives. They describe man as he does things. These adjectival qualifications belong to man and describe him, not ontologically but historically.

The ontological dimension of man transcends all adjectival qualifications. The adjective can only describe one aspect of the ontological dimension but cannot exhaust it or identify itself with it. The dimension of being transcends all other dimensions and each adjective can bring forth only one aspect of being. It is this fact which limits each adjectival qualification and points to the inexhaustible nature of being.

The adjectives give us this statement: man (in his historical dimension) is political, economic, and social. The "other" of the ontological, the adjective, is necessarily implied in the ontological, because being the "other" it allows the ontological to measure its dimension as "other" than the adjective. Being "other" than the adjective it reveals that it is not the adjective or can be adjective, but at the same time only through the adjective does it know itself to be "other" and itself at the same time. The same reason can be ap-

plied to the adjective. The adjective can be conscious of its dimension because the "other" is the ontological dimension. The adjective measures itself in relation to its "other" becoming conscious of itself and its "other" at the same time and being conscious of itself as constituting the "other" of the ontological. With this relation we arrive at an understanding which points to the fact that neither the ontological nor the practical in themselves can account for man. A definition of man is possible only when we understand that in a statement such as "Man is political" both the noun and adjective are seen to reveal the verb, is. This occurs when the ontological meaning of man measures its dimension in relation to its "other," the adjective which belongs to the ontological dimension by virtue of the fact that this dimension knows itself through the adjective. The same holds for the adjective. If the adjective is considered apart from the noun it could not reveal itself to itself, but the noun being the "other" of the adjective is at the same time the reality through which the adjective limits its own dimensions and reveals itself. To speak of adjective or noun in other but related terms, i.e., implying each other, is to deny the truth that the meaningfulness and significance of each lies not in its own form but its own form comes to a consciousness of itself through its other. The content of each term finds its necessary form in relation to the other. Thus man and political reveal the verb, is; neither one alone can accomplish this. Realizing their particular dimensions through each other they state something about the human dimension. The human dimension is neither political nor man, it is both, each possessing individuality and at the same time the individuality of each achieves its form and dimension through the other. Heidegger's attempts to explain the meaning of man within the ontological dimension can be achieved only if he also attempts to find the relationship between the ontological dimension and the practical dimension. To disregard the practical dimension is to hinder the full realization of the ontological dimension.

As an introduction to Heidegger's thought the book is successful. Heidegger's vocabulary and linguistic structure are carefully explained and interpreted. The book, however, never goes beyond its introductory character. The analysis and evaluation of Heidegger's thought is again only an introduction to an analysis and an evaluation. This volume requires a second, a careful interpretation of the consequences of the strengths and weaknesses inherent in Heidegger's philosophy.

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